V. "An Account of Experiments made at the Kew Observatory for determining the true Vacuum- and Temperature-Corrections to Pendulum Observations." By Balfour Stewart, Esq., F.R.S., and Benjamin Loewy, Esq., F.R.A.S. Received May 27, 1869.

1. Pendulum-observations, whether undertaken for the purpose of obtaining unalterable standards of length or for physical and geodetic objects, are usually made in air or in a receiver, from which the air is partially or almost entirely withdrawn; and in order to render such observations, made at different places and by different observers, capable of intercomparison, they are, by means of a "correction for buoyancy," reduced to a vacuum. It is well known that the most illustrious physicists and mathematicians have given a great deal of attention to a correct determination of the principles on which this reduction to a vacuum ought to be based, and of the actual resistance which such a body as a pendulum meets during its vibrations in a fluid body. Until some years ago, especially since the researches of General Sabine and Bessel, it was thought best to determine for every pendulum a certain constant by finding its vibrations in air at the usual pressure, and also in a receiver from which the air is as much as possible withdrawn; from the difference in the number of vibrations thus found the correction was then calculated on the assumption that this difference is proportional to the difference of density of the air.

2. In the pendulum-observations made at the Kew Observatory in connexion with the Great Trigonometrical Survey of India (*vide* Proceedings of the Royal Society for 1865, No. 78) we adopted, for determining the necessary constant, the method first carried out by General Sabine, and of which a detailed account is given in the Philosophical Transactions for 1829, Part I. page 207 &c. But since our account has been published, two eminent physicists, Professor Clerk Maxwell and Professor O. E. Meyer in Breslau, have independently investigated the internal friction in gases, and its effect upon bodies moving in them; and among the prominent results obtained by them is this, that the influence of the internal friction of a fluid on a moving body is not proportional to its density. However, for small differences of pressure, such as those experienced by General Sabine in his researches, the old method for determining the correction is sufficiently accurate; or again, if a series of such experiments as our own fundamental Kew observations for India be made at a very low pressure, say from $\frac{1}{2}$ an inch to $1\frac{1}{2}$ inch, the correction is itself a very small quantity; and the application of a more correct principle of reduction will not sensibly affect the ultimate results, because the difference between the true and approximate correction is in such a case extremely small. But if, as is the case in the Indian observations, experiments are made at higher and varying pressures, it is very desirable to apply experimental methods which will give the true correction.

3. With a view to collect for the theory of the subject a great many

carefully conducted experiments, and also to supply those who are actually engaged in pendulum-experiments at the present time with practically valuable results, we proposed to ourselves to observe the behaviour of pendulums of the different forms hitherto used in such researches in which the pendulum is employed, at pressures varying through the whole range, from the lowest obtainable in a receiver to the usual atmospheric pressure. The carrying out of our intentions met, however, with many delays through unavoidable circumstances, and there is, indeed, at present little prospect of our being able to complete the whole of the original plan. We give, therefore, here an account of some preliminary results which are, in our opinion, not without practical importance, and which will certainly find their use in the reduction of observations made with pendulums of a form similar to that used by ourselves, viz. that form of reversible pendulum known as "Kater's pendulum."

4. The following is an account of the operations:—The pendulum was swung in the Kew receiver, made of five pieces, two of metal and three of glass, the parts fitting closely, and the whole being connected with siphon-gauge and air-pump by tubes. One of the metal pieces is perforated behind and in front, and the apertures are covered by plate glass for the observation of the coincidences.

The pendulum was swung at the following pressures:—

I. At about $\frac{1}{2}$ of an inch (lowest obtainable).	V. Between 4 and 5 inches.	X. At about 20 inches.
II. Between 1 and 2 inches.	VI. „ 5 „ 6 „	XI. „ „ 25 „
III. „ 2 „ 3 „	VII. „ 7 „ 8 „	XII. At the full atmospheric pressure.
IV. „ 3 „ 4 „	VIII. At about 10 inches.	
	IX. „ „ 15 „	

At each pressure a good many observations were made, in order to ensure reliable mean results.

5. With reference to the *registration of the observations*, we have strictly adhered to the method previously adopted after careful consideration, and explained in our former account; hence we need not here enter upon this part again. Instead of registering *one* coincidence at the beginning, during the progress, and at the end of an experiment, we have this time in most cases observed *three* successive coincidences, and the arithmetical mean of these, together with the mean of the corresponding registrations of arc, temperature, and pressure, stands for one observation; we think that this method ensures greater correctness, although it is more laborious than that previously adopted.

6. The *reduction of the observations* comprises, as shown in the previous paper alluded to above, several corrections to be applied to the number of observed vibrations; we shall mention here only those points which differ numerically or experimentally from the numbers or methods explained in that paper, which contains also an experiment with its full reductions. By referring to these and the following remarks our method of procedure will be so abundantly clear, we hope, that we shall be able to proceed immediately afterwards to the statement of the final results.

A. *Correction of the observed arc-readings and reduction of the vibrations to infinitely small arcs.*

We have previously shown that if

D=distance of scale from the object-glass of the telescope,
d=distance of scale from the tailpiece of the pendulum,
O=observed scale-reading for the whole arc of vibration,
S=distance of indicating-point of the tailpiece from the knife-edge,
α=true semiarc of vibration,

$$\text{then } \tan\alpha=\frac{O(D-d)}{2DS},$$

expressing all distances in inches into which the scale is divided.

In our case repeated measurements gave the following mean values for these quantities:—

$$D=101{\cdot}86 \text{ inches}; \; d=0{\cdot}56 \text{ inch}; \; S=47{\cdot}55 \text{ inches}.$$

Hence we have to add $\log\left(\frac{D-d}{2DS}\right)=\log\left(\frac{101{\cdot}3}{2\times101{\cdot}86\times47{\cdot}55}\right)=\bar{2}{\cdot}0194252$ to the logarithm of the observed scale-readings to obtain the semiarc of vibration.

For the reduction to infinitely small arcs, we have again used the well-known formula number of infinitely small vibrations

$$=n+n.\frac{M\sin(\alpha+\alpha')\sin(\alpha-\alpha')}{32(\log\sin\alpha-\log\sin\alpha')},$$

the symbols having the same meaning as previously stated.

We are well aware that more convenient formulæ, and more correct methods, have been used or proposed by different observers for this correction; but we thought it best to adhere to a uniform method in the reductions, in order to facilitate any future rediscussion of our original observations (which are preserved at the Kew Observatory), should such appear desirable when the results of the Indian pendulum observations will be published.

B. The precise determination of the *rate of the clock* might have been of minor importance in our experiments, an approximate uniformity of rate being the chief desideratum. We expected, however, that very small differences in the number of vibrations would result in those experiments where the pressure differed only by an inch or even less. We considered it hence of the utmost value to have a precise record of the behaviour of the clock during these experiments, so as to discover at once changes in the rate, and to make our reductions depending on it for each experiment. A great number of transit-observations were accordingly made, and during these not only the pendulum-clock, but also the behaviour of a chronometer by Dent and a meantime-clock by Shelton was accurately determined. These two latter served during those days when no transits could be obtained for deducing the rate of the pendulum-clock by intercomparison. From the whole of these observations the following Table of the number of vibrations during a mean solar day has been calculated for every day of

the four months during which the experiments were carried on, each result being of course employed for the pendulum-experiments of the corresponding day. The Table shows that our plan was the safest, as differences of nearly one second are observable; these differences have, however, apparently no connexion with changes of temperature, as the rate of the clock during the artificial heating of the pendulum-room, of which we shall soon have to speak, showed hardly any difference from the mean rate, proving that the compensation was not faulty.

The pendulum-clock showing sidereal time, the Table is calculated from the formula:—Number of vibrations in a mean solar day $=N'=86636{\cdot}5554\left(1-\frac{r}{86400}\right)$; and hence: Number of vibrations of detached pendulum $=N=\frac{VN'}{V'}$, where V, V' are respectively the number of vibrations of the detached and clock-pendulum from beginning to the end of our experiment.

TABLE I. *Number of Vibrations made by the clock-pendulum during a mean solar day.*

1866.	September.	October.	November.	December.
1.	76577·40	86577·38	86577·43	86577·50
2.	·31	·25	·32	·40
3.	·50	·14	·20	·39
4.	·29	·09	86576·99	·48
5.	86576·97	·21	86577·02	·31
6.	·83	·22	86576·94	·39
7.	·81	·06	·95	·50
8.	·88	86576·91	·99	·54
9.	·90	86577·00	86577·11	·54
10.	86577·04	·11	·17	·51
11.	·00	·13	·24	·61
12.	·07	·28	·33	·68
13.	·26	·10	·28	·70
14.	·16	·04	·40	·70
15.	·11	·22	·53	·64
16.	·08	·29	·61	·64
17.	86576·98	·40	·60	·60
18.	·90	·50	·70	·70
19.	·70	·54	·75	·71
20.	·90	·60	·71	·66
21.	·99	·61	·80	·59
22.	·93	·43	·74	·40
23.	·85	·28	·70	·47
24.	·83	·27	·62	·44
25.	·99	·47	·51	·46
26.	86577·20	·40	·49	·48
27.	·21	·29	·44	·47
28.	·33	·33	·64	·59
29.	·30	·35	·56	·61
30.	86577·24	·29	86577·56	·51
31.	....	86577·40	....	86577·56

C. *Correction for temperature.*—The method of suspending the ther-

mometers was precisely the same as that used in our previous experiments and described in the account we gave of them to the Royal Society. The formula employed for deducing the most probable mean temperature for each experiment was as before, using the same symbols :—

$$t^{\circ}=\frac{n\left(\frac{t+t'}{2}\right)+n'\left(\frac{t'+t''}{2}\right)+n''\left(\frac{t''+t'''}{2}\right)\ldots\ldots}{n+n'+n''\ldots\ldots}$$

As explained in the paper mentioned above, we had then no time at our disposal to swing the pendulums used at extremes of temperature, and hence we availed ourselves of the elaborate series of experiments on the temperature-correction of pendulums made by General Sabine (*vide* Phil. Trans. 1830, p. 251), adopting the mean of his entire results, viz. 0·435 vibrations per diem, as correction for 1° of Fahrenheit's scale for our reductions.

In our present investigation we thought it indispensable to obtain the utmost accuracy, by ascertaining the temperature-correction for each pendulum intended to be used by an independent series of experiments in an artificially heated room, the natural annual range of temperature in the Kew pendulum-room being insufficient for our purposes. The arrangement consisted in erecting an iron stove in the vicinity of the pendulum-apparatus, and carrying a long pipe through the whole height of the room. By several preliminary trials, it was soon found that up to about 80° the temperature could be maintained constant for several hours, but that the difficulties increased with the rise of the temperature, and became almost unsurmountable when the temperature was above 100°. Besides the maintenance of a pretty equable temperature during the duration of an experiment, another difficulty arose. The pillar of masonry which carries the apparatus on one side, and the wall of the room on the other, prevented us giving to the heating-apparatus such a lateral position as to bring the bar which carried the thermometers and the pendulum in equal proximity to it. The stove had to be placed in front, somewhat to the left of the apparatus, and hence the brass bar which carried the thermometers was nearer to the source of heat than the pendulum. In order to arrive at the most exact measurement of the real temperature of the pendulum, two additional thermometers were suspended behind it, at about the same distance from it as those in front, and all four were read during the experiments.

Seeing from the preliminary trials that an approximately equal distribution of temperature throughout the apparatus could only be relied upon up to about 70°, and that after that point the differences in the readings of the thermometers, behind and in front, increased to an extraordinary degree, we decided upon making two different classes of experiments, viz. one set confined to temperatures of about 70°, and another comprising higher temperatures; and we further, during the pro-

cess of reducing our observations, came to the conclusion that it would be best to exclude all experiments made at temperatures above 100°, and also those where great differences in the readings for temperature occurred, from our final results. The principle which guided us was not to vitiate good observations by doubtful ones ; and the following small Table, showing the temperature-readings during four experiments, taken quite at random, will show best how we proceeded:—

I.					II.				
Thermometer in front.		Thermometer behind.			Thermometer in front.		Thermometer behind.		
Upper therm.	Lower therm.	Upper therm.	Lower therm.		Upper therm.	Lower therm.	Upper therm.	Lower therm.	
71·20	70·20	70·0	68·5	Experiment good.	85·6	83·7	84·8	83·5	Experiment good.
71·15	70·20	70·4	69·4		86·1	83·8	84·0	83·0	
71·30	70·00	70·6	69·7		86·7	83·6	83·3	82·6	
71·85	69·60	71·4	71·4		78·4	78·8	82·2	82·1	
72·00	69·55	71·4	71·6		79·4	80·4	80·8	81·2	
71·90	69·50	71·4	70·7		81·2	82·0	79·4	82·8	
Mean 70·9		70·6			82·5		82·45		
70·8					82·5				
III.					**IV.**				
99·5	98·7	92·4	91·5	Experiment rejected.	108·4	107·3	100·6	99·5	Experiment rejected.
97·4	97·0	91·6	90·6		107·6	106·5	99·7	99·0	
95·5	94·3	90·3	88·1		105·7	103·8	98·5	97·3	
82·5	79·9	78·5	76·2		97·4	96·5	93·1	91·3	
81·6	79·3	77·4	76·0		96·3	95·1	92·2	90·9	
80·9	79·2	75·5	74·1		95·9	94·7	91·8	90·3	
Mean 88·8		83·7			101·3		95·3		
86·2					98·3				

Although in the rejected experiments the means of all readings of both sets of thermometers approach each other, still there occurs a fall of nearly 20° at the end of an experiment, as compared with that temperature which is recorded at the beginning, besides differences of nearly 8° between the thermometers in front and those at the back of the pendulum. That the temperature of the latter during an experiment is represented by the arithmetical mean of such discordant readings, we think most unlikely ; and hence these experiments and similar ones were not used, although, of course, the number of available experiments was thereby reduced.

The following experiments, which represent the final results of this temperature-investigation, deserve at least some confidence, although we

should have liked to see their number much increased. To avoid a correction for pressure, we took care to correct at once, before beginning an experiment, the reading of the gauge to 32° of temperature, and to regulate by a few strokes of the pump the pressure, so as to assimilate it to the mean pressure (also reduced to 32°) of the experiments previously made in cold air. All pressures are reduced to 32°, and the small difference of pressure which still resulted, comparing the mean of the hot-air experiments with those in cold air, amounting to about $\frac{1}{100}$ of an inch, has been disregarded in the final reduction.

An attempt to test the constancy of the temperature-correction *in vacuo*, with reference to a suggestion made by Colonel Walker, Superintendent of the Great Indian Survey, who suspects that the coefficient of expansion of a pendulum in air varies slightly from that in a vacuum, proved a failure. The pomatum which is used for tightening the different parts of the receiver melted by the heat of the stove, and rendered it impossible to reduce the pressure in the receiver sufficiently for the purpose of the experiments.

I. Experiments made in cold air.				II. Experiments made in hot air.				
No. of exp.	Temperature.	Pressure.	No. of vibrations per day.		No. of exp.	Temperature.	Pressure.	No. of vibrations per day.
	°	inches.				°	inches.	
1.	47.84	30.052	86013.60	First set.	1.	70.6	29.964	86002.72
2.	47.77	30.112	86013.76		2.	71.3	29.970	86002.64
3.	46.33	30.182	86013.82		3.	72.1	29.958	86002.50
4.	46.25	29.938	86014.02		4.	70.8	29.950	86002.90
5.	47.72	30.460	86013.58		5.	73.8	29.960	86002.43
6.	47.91	29.498	86013.64	Second set.	1.	82.5	29.914	85997.84
7.	48.43	29.582	86013.98		2.	82.5	29.988	85997.10
8.	47.76	29.328	86014.40		3.	83.9	29.960	85995.65
9.	45.09	30.202	86013.90		4.	88.3	29.941	85992.23
10.	46.19	30.011	86013.99		5.	90.8	29.982	85992.73
11.	47.50	30.107	86013.61		6.	99.9	29.971	85988.43

The following are the *mean results*, with their respective *differences* :—

	Temp.		Pressure. inches.		Vibrations.
	°				
A. Experiments in cold air	47.16	......	29.952	......	86013.85
B. Experiments in hot air, first set	71.64	......	29.960	......	86002.64
C. Experiments in hot air, second set ...	88.00	......	29.959	......	85994.00

Resulting differences of temperature and number of vibrations :—

		Temperature.		Vibrations.
		°		
$A \sim B$	=	24.48	......	11.21
$A \sim C$	=	40.84	......	19.85
$B \sim C$	=	16.36	......	8.64

Hence we find a correction between

$$\left.\begin{array}{l} 47^{\circ}\!\cdot\!16 \text{ and } 71^{\circ}\!\cdot\!64 \text{ of } \frac{11\cdot21}{24\cdot48} = \cdot458 \text{ vibrations} \\ 47\cdot16 \text{ „ } 88\cdot00 \text{ „ } \frac{19\cdot85}{40\cdot84} = \cdot486 \text{ „} \\ 71\cdot64 \text{ „ } 88\cdot00 \text{ „ } \frac{8\cdot64}{16\cdot36} = \cdot528 \text{ „} \end{array}\right\} \begin{array}{l}\text{per diem for one}\\ \text{degree of Fahr-}\\ \text{enheit's scale.}\end{array}$$

Comparing these results with those obtained by General Sabine (Phil. Trans. 1830, p. 251, &c.), we find that the pendulums employed by him gave a correction of 0·44 of a vibration per diem for each degree of Fahrenheit between 30° and 60°, a result which agrees well with that found by ourselves between 40° and 70°, the small difference being probably referable to a difference in the composition of the metal of which the pendulums were made. But a considerable difference appears in the experiments made at the higher temperature. General Sabine made some experiments, previously to those discussed in the above-mentioned paper, with two different pendulums in a chamber artificially heated to between 80° and 90°, which gave for the correction for each degree of Fahrenheit, respectively for the two pendulums, 0·432 and 0·430 vibrations, corresponding to that part of the thermometer-scale which is included between 45° and 85°. These results are somewhat different from those which are obtained for the scale-reading between 30° and 60°, and General Sabine points to this difference in the following words * :—

"In the experiments in the chamber artificially heated, the fluctuations of temperature, in spite of every precaution, were considerable, and rendered the determination of the mean temperature more difficult, and probably less exact than in the natural temperatures; hence it would be unsafe to conclude in favour of the inference to which these facts would otherwise lead, that the correction at high temperatures is less than at low temperatures, or that the metal expands a smaller proportion of its length for one degree between 85° and 45° than for one degree between 60° and 30°."

Our own experiments, on the other hand, seem to agree with the general fact that the coefficient of expansion increases with the temperature, and that in a series of experiments a lower range of temperature will give a lower, a higher range a greater value for the expansion for one degree. Nevertheless the values resulting from our high temperature-experiments appear decidedly too large to be explained solely by this general behaviour of bodies; and in our reductions of the pressure-experiments, where the differences of temperature, as will be seen in the following paragraph, are inconsiderable, we have adopted that value for the temperature-correction which results from the experiments between 45° and 70°, viz. 0·458 of a vibration for one degree, a result which not only well agrees with those found by General Sabine, but also appeared to our

* Phil. Trans. 1830, p. 252.

own considerations the most reliable, for reasons which will appear presently.

Speaking generally of the subject of the temperature-correction, we must admit that our experiments do not tend to remove the difficulties that seem to surround it. Our experience goes to prove, what the Indian officers, entrusted with the pendulum-experiments and their reduction, have also suspected, that the thermometers fixed to a so-called dummy-bar (in order to place them in conditions similar to the swinging pendulum) do not give a true indication of the real temperature of the pendulum. If this is the case, the differences found by ourselves between the result of the lower and that of the higher range can easily be understood. Indeed, during the progress of these experiments it has always appeared to us that not only the fluctuations indicated by the thermometers are *greater* in range than those to which the pendulum itself is subjected, but that they are also more rapid, and that the heavy and substantial pendulum cannot keep time in these changes with the light and delicate thermometers which are not absolutely sealed up into the substance itself. In our experiments, each of which lasted from one to two hours, a high temperature was usually produced at the beginning, and we attempted to maintain the heat as much as possible by keeping the pendulum-room closely shut on all sides during the progress of the experiment. The inrush of cold currents can, however, obviously not be wholly prevented, and a steady, more or less considerable fall of the temperature is recorded in each of the experiments beyond 70°. This fall affects, in our opinion, chiefly the thermometers themselves, while probably the pendulum maintains its higher temperature much longer. Thus we are inclined to think that the mean temperature of the pendulum, if it could by some means be exactly ascertained, might perhaps appear considerably higher than the mean of the thermometer-readings recorded; and to this circumstance we ascribe it mainly that the high temperature-experiments give too large a correction, for in these experiments a greater difference in the number of vibrations corresponds to an apparently smaller difference in temperature.

The question will, we have reason to hope, find its best solution by the labours of Colonel Walker and Captain Basevi in India, where these gentlemen can avail themselves of a great natural range, which will free the experiments from the doubts and difficulties met by ourselves; but we cannot conclude this part without reminding experimenters of the words with which, nearly forty years ago, General Sabine concluded the account of his own experiments, and which have gained new force by the shortcomings of our own investigations *.

"It seems therefore desirable, for the sake of experiments, which are becoming greatly multiplied, and which are daily increasing in accuracy, that means should be devised of obtaining the rates of pendulums in

* Phil. Trans. 1830, p. 253.

artificial temperatures, embracing a wider range than the natural temperatures, but capable of being determined with equal accuracy."

7. There remains now only to give the results of the experiments made for determining the changes in the number of vibrations of our pendulum produced by varying pressures, and hence the correction necessary to reduce experiments made at any pressure to a vacuum. These results, as given by each separate experiment, are contained in the following Table :—

TABLE II. *Experiments for determining the number of vibrations made by Kater's invariable pendulum at different pressures.*

No. of experiment.	Full atmospheric pressure.			About 25 inches.			About 20 inches.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inches.		°	inches.	
I.	47·84	30·052	86013·60	45·82	24·632	86014·57	47·66	19·895	86015·87
II.	47·77	30·112	86013·76	45·54	24·634	86014·70	47·21	19·852	86015·99
III.	46·33	30·182	86013·82	46·03	24·620	86014·71	51·04	19·902	86016·01
IV.	46·25	29·938	86014·02	47·19	24·599	86014·49	48·00	19·914	86016·07
V.	47·72	30·460	86013·58	51·01	24·651	86014·60	46·47	19·921	86016·10
VI.	47·91	29·498	86013·64	49·84	24·646	86014·70	49·00	19·864	86015·94
VII.	48·43	29·582	86013·98						
VIII.	47·76	29·328	86014·40						
IX.	45·09	30·202	86013·90						
X.	46·19	30·011	86013·99						
XI.	47·50	30·107	86013·61						

No. of experiment.	About 15 inches.			About 10 inches.			Between 7 and 8 inches.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inches.		°	inches.	
I.	43·47	14·563	86017·91	50·75	9·998	86019·65	49·37	7·586	86021·61
II.	44·30	14·567	86017·84	45·54	9·868	86019·29	54·11	7·491	86021·41
III.	50·07	14·532	86018·20	47·13	9·900	86019·43	51·29	7·303	86021·30
IV.	51·71	14·680	86017·95	48·24	9·870	86019·60	50·06	7·554	86021·44
V.	47·09	14·575	86018·00	49·01	10·015	86019·35	53·42	7·601	86021·19
VI.	46·88	14·499	86017·95	51·77	10·064	86019·51	48·73	7·384	86021·58
VII.	54·33	14·555	86017·93						

No. of experiment.	Between 5 and 6 inches.			Between 4 and 5 inches.			Between 3 and 4 inches.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inches.		°	inches.	
I.	52·13	5·461	86022·10	51·07	4·241	86022·48	51·34	3·144	86023·03
II.	47·86	5·420	86022·11	51·17	4·245	86022·71	50·95	3·155	86022·90
III.	48·90	5·510	86021·97	54·06	4·440	86022·69	50·80	3·266	86022·94
IV.	51·30	5·403	86021·84	50·39	4·530	86022·69	52·09	3·204	86022·70
V.	49·63	5·390	86022·19	49·84	4·107	86022·37	54·16	3·170	86022·71
VI.	54·17	5·489	86022·15	54·77	4·298	86022·54	53·70	3·104	86022·95

TABLE II. (*continued*).

No. of experiment.	Between 2 and 3 inches.			Between 1 and 2 inches.			Below 1 inch.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inch.		°	inch.	
I.	51·50	2·373	86023·23	60·97	1·393	86023·35	62·76	0·472	86023·26
II.	48·93	2·462	86023·09	59·47	1·416	86023·24	60·85	0·431	86023·47
III.	55·55	2·501	86023·05	58·32	1·411	86023·04	60·79	0·444	86023·60
IV.	54·76	2·389	86023·21	61·15	1·430	86023·45	61·57	0·451	86023·74
V.	52·38	2·417	86023·08	60·83	1·471	86023·65	62·36	0·425	86023·79
VI.	54·14	2·451	86023·23	57·58	1·319	86023·17	60·77	0·389	86023·31
VII.	......	......	....	......	......	....	49·72	0·427	86023·55

TABLE III. *Mean results of Pressure-experiments.*

	Mean pressure. inches.		Mean number of vibrations per diem.		Mean pressure. inches.		Mean number of vibrations per diem.
I.	0·434	......	86023·53	VII.	7·486	......	86021·42
II.	1·407	......	86023·32	VIII.	9·953	......	86019·47
III.	2·432	......	86023·15	IX.	14·569	......	86017·97
IV.	3·174	......	86022·87	X.	19·891	......	86016·00
V.	4·310	......	86022·58	XI.	24·630	......	86014·63
VI.	5·445	......	86022·06	XII.	29·952	......	86013·85

while Table III. contains the resulting means for the different sets, as specified in paragraph 4.

The experiments made at a full atmospheric pressure are the same as those given previously in connexion with the temperature-experiments, but they are here repeated for the sake of comparison. Their mean temperature being 47°·16, the whole of the other experiments has been reduced to the same temperature by means of the coefficient adopted in accordance with our preceding statement.

The results as given in Table III. do not require any special remarks. It will be seen that the resistance of the air to the motion of a pendulum, as measured by the number of its vibrations, increases very slowly up to 7 or 8 inches of pressure; a more energetic action is exerted up to about 20 inches, and after that point the resistance increases very slowly up to the full atmospheric pressure.

This behaviour is represented in a more impressive manner on the accompanying curves. One of them, marked A, shows simply the resulting number of vibrations at the given pressures, which latter form the abscissæ, while the former are the ordinates. The second curve, B, is derived from A, by assuming the whole correction necessary to reduce the pendulum observations made in air to a vacuum as unity, and expressing the correction for intermediate pressures as fractions. The ordinate representing unity has been divided into forty parts, each representing 0·025, enabling us to represent the correction to three decimals with great precision.

The straight dotted line, C, gives the old correction, and shows best how it differs from the correct one.